In cases where it is impossible to give the magnitudes, the three last figures will be written as three zeros.

This alteration will come into use on the first of next month (November) in all telegrams from the "Centralstelle" in Kiel.

COMETS PERRINE (1895 IV.) AND PERRINE-LAMP (1896).—A most interesting description of these two comets, obtained from eye observations and photographs, is given by Joseph and Jean Fric, in a communication presented on April 24 of this year to the Cisaře Františka-Josefa (5th year, No. 26). Up to the time of the perihelion passage of Comet Perrine, the observations made during this period were published in the *Bulletin* of the same Academy (No. 8), the last observation dating from December 9. The path of the comet at and since the time of its perihelion passage, is here indicated in the chart accompanying this communication. In the cliché taken on February 15 of this year, the tail of this comet appears in the form of a thin line, with a position angle of 120°, being turned towards the sun. This latter exceptional fact has been verified on two negatives taken on February 20 and 21, both of which were made under the best atmospheric conditions. The description of the original clichés that were taken on February 15, shows us that this comet presented a very dim line directed towards the sun, and of 1° in length. The nucleus was nearly of fifth magnitude, and resembled a star. This cliché is further interesting from the fact that it shows the first trace of the new comet Perrine-Lamp. On February 20 the tail presented a fan-like form, being somewhat more dense at the position angle 120°. Its breadth was 15′ at its centre, and its length 1½°. The position of the tail was abnormal, being turned towards the sun. By April 21 its length had increased to 2°. On March 15 only the nucleus was visible, and by the 20th a photograph showed only a feeble trace of it. The clichés which show the appearance of the Perrine-Lamp comet are also full of interest. A striking feature of these photographs is the bifurcation of the tail, exhibited on the cliché made on February 22 and March 3, and its spontaneous development on February 21 and 22. The direction of the tail, in its relation to the sun, was normal. The communication contains, besides the chart referred to above, reproductions of the several clichés mentioned in the pamphlet.

THE CANALS OF MARS.—First Schiaparelli and then Lowell have both shown us that the Martian surface is a network of canals. The number of canals, as the latter observer informs us, is really far more numerous than has yet been recorded, but these are less in size, and only flash out clearly under the very best and exceptional conditions of seeing. As one would expect, the greater the number of canals, the greater becomes the difficulty in identifying them. In fact, unless one has first-class conditions for observation, and also considerable experience, it is rather rash to suggest the discovery of new canals. Mr. Brenner seems, however, to be certain of his powers of identification, and describes some of his observations in the *Bulletin de la Société Astronomique de France* (October). Without diagrams it is unsatisfactory to try to describe the positions of these suspected new canals, but a reference to Mr. Lowell's chart seems to indicate that these may be cases of not exact identification. Mr. Brenner makes it very difficult for readers of his notes, as he inserts woodcuts of the surface markings, numbered most carefully, these numbers having no reference at all to the text. For instance, referring to one of the drawings he says: "One sees the following canals: (1) Steropes, (2) Glaucus, (3) Phlegethon, (4) Cérannius, &c."

As these are the only numbers used in the text, it is natural to suppose them to refer to the illustrations; this, however, is far from the case, hence the delusion.

An interesting point is touched upon by Prof. V. Cerulli, concerning the conspicuousness of the canals Ulysses and Sitacus. These canals are not charted by Schiaparelli, but were discovered by Lowell two years ago Prof. Cerulli asks the question, How is it that they have been previously not seen, considering that the former is now as prominent as Sirenius and Araxes, both in the chart of Schiaparelli, and that the latter surpasses in distinctness the Euphrates and Phison? They are not simply canals that were observed in 1894 for the first time, but they are canals which till then had no existence. Mr. Lowell also remarked a peculiarity in this respect. Referring to the canals not on Schiaparelli's chart (Lowell, "Mars," p. 148) he says: "The most peculiar case, however, is the relative conspicuousness of the Ulysses."

THE HUXLEY LECTURE.—RECENT ADVANCES IN SCIENCE, AND THEIR BEARING ON MEDICINE AND SURGERY.[1]

II.

NOW let me turn to another theme suggested by what has happened in science and in the profession since the days of Huxley's studentship, and that is the complexity of the bearings of any one discovery, of any one advance, as well on science itself as on the applications of science.

In the garment of science, with which man is wrapping himself round, or rather is being wrapped round, the several threads are woven into an intricate web. As the loom which is weaving that ever-spreading garment takes in new warp and new woof, such threads only of each are taken in as can be fitly joined to those which have come in before, each thread as it is twisted in becomes a hold for other threads to be caught up later on. No single observation, no single experiment stands alone by itself, nor can its worth be rightly judged by itself alone. The mistaken philanthropists who have put restrictions, and would put more on physiological investigations, betray that ignorance of the ways of science, which seems to be a necessary condition of their attitude, when they ask us to state in a sentence the direct application to the good of man of each experiment on a living animal. In the doors of science, each the opening as often of a path as of a chamber, it is not, as such folk seem to think, that each bobbin pulls only one latch. Every experiment, every observation has, besides its immediate result, effects which, in proportion to its value, spread away on all sides into even distant parts of knowledge. The good of the experiment by itself is soon merged in the general good of scientific inquiry. The science of physiology, and by implication the art of medicine, is built up in part on experiments on living animals; in part only, but that part is so woven into all the rest that any attempt to draw it out would lead to a collapse of the whole.

It is because each experiment or observation is thus a thread caught up in a close-set web, that its value depends not alone on the mere result of the experiment or observation itself, but also, and even more so, on the time at which, and on the circumstances and relations under which it is made. This truth the real worker in science has borne in upon him again and again; it is this which leads him to that humility which has ever been the outward token of the fruitful labourer. He feels that it is not so much himself working for science as science working through him.

Let me attempt to illustrate this by dwelling on some two or three single observations in physiology, made almost at the time, or very soon after the time at which Huxley was a student. It will, I think, be seen that each of them has reached a long way in its bearing on the science of physiology and on the art of medicine, that the full effect of each has been dependent both on what went before and on what has happened since, and though they were all made, so to speak, long ago, some of their fruits were brought in as it were yesterday, and their full fruition is perhaps not yet accomplished.

I will first invite your attention to a single experiment, for, though repeated on various animals, we may call it a single experiment, which in the fall of the year 1845 Ernest Heinrich Weber, then Professor of Anatomy at Leipzig, and his brother Eduard Friedrich, reported to an assembly of Italian scientific men in Naples, and of which they subsequently published an account in Müller's *Archiv* in 1846. Making use of the recently introduced rotating electro-magnetic apparatus (the physical discovery begetting the physiological one), they found that powerful stimulation of the vagus nerves had the unexpected result of stopping the heart from beating.

This single experiment, which I may quote by the way as a typical experiment on a living animal—for it is impossible to imagine how the discovery of this action of the vagus on the heart could have been made otherwise than by an experiment on a living animal—this single experiment has made itself felt far and wide throughout almost the whole of physiology.

In the first place, it has made us understand in a way impossible before the experiment, how through the intervention of the nervous system, the work of the heart is tempered to meet the strain of varying circumstances. As I said a little while back, only a few years before even eminent observers were groping about in a dim light, hotly discussing whether the brain and spinal cord could affect the beat of the heart. To all these discussions Weber's experiment came as a great light in a dark place.

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[1] Delivered at Charing Cross Medical School, on October 5, by Prof. Michael Foster, Sec.R.S. (Continued from p. 583.)

There is no need for me to insist how this knowledge that impulses descending the vagus slow or restrain the heart beat, and the knowledge genetically dependent on this, that impulses reaching the heart along the cardiac sympathetic nerves from the thoracic spinal cord, stir up the heart to more vigorous or frequent beats, have since served as a guide for the physician in the intricate problems of cardiac disease, and that with increasing security as our knowledge of the details of the actions has increased. The knowledge may not always have been wisely used. On this point perhaps I may be allowed to repeat the caution which I may have given elsewhere, concerning the dangers of taking a new physiological fact direct and straight, raw and bleeding as it were, from the laboratory to the bedside. The wise physiologist takes care, even in physiology itself, not to use a new fact as an explanation of old problems without a due testing and a direct verification of its applicability. How much more is it needful that the doctor who sails not on the calm seas of the phenomena of health, but amid the troubled tempests which we call disease, should not hastily and heedlessly rush to make practical use of a new fact, tempting as the use may be, until he also has tested its applicability by that clinical study which is his only sure guide. But this is by the way.

In the second place, as a mere method, Weber's discovery has in physiological experimentation borne most important fruit. Before Weber's experiment many an investigation, not only on the vascular system itself, but in many other branches of physiology, came to a standstill or went astray because the experimenter had not the means on the one hand to stop or slacken, or on the other to quicken and stir up the heart, without interfering largely with the object of his research. Thanks to Weber's experiment and what has come out of it, that can now be done with ease, and thus solutions have been obtained of problems which otherwise seemed insoluble.

In the third place, the experiment has had a profound and widespread influence by serving to introduce a new idea, that idea which we now denote by the word inhibition. Before the experiment, though men's minds were gradually getting clearer concerning the nature of a nervous impulse, all known instances of the action of a nervous impulse had for the result an expenditure of energy; and it was a still open, though hotly debated question whether in such actions as when a muscle was thrown into contraction by a nervous impulse this feature of expenditure was not impressed on the muscle by the very nature of the impulse itself. That question the experiment answered in the negative once and for all. Whatever the exact nature of a nervous impulse, it was evidently of such a kind that it might on occasion check expenditure and bank up energy in an increased potential store. Observation soon showed that the heart and vagus was no solitary example. It was recognised that the due regulation of many of, if not all, the so-called nervous centres was secured not merely by the intrinsic forces of passive rest making themselves felt in the absence of stimulation, but also, and even more so, by the alternating play of antagonistic influences. Throughout all the sciences the resolving a stability seemingly due to intrinsic causes into an equilibrium arising out of the balance of opposing forces, has again and again marked a step forward; and it is perhaps not too much to say that a like analysis, prompted by the story of the vagus and the heart, has profoundly modified all our conceptions of the way in which nervous impulses, sweeping along the intricate yet ordered network of paths in the brain and spinal cord, determine the conduct of life. The idea has of course been abused as well as used, as what idea has not? Such a word as inhibition could not but fail to have a blessed sound in the ears of the ignorant; the idea has been ignorantly and wrongly applied; but this is of little moment in view of the help which it has given to wise and well-directed inquiry.

And the idea has spread with fruitful results beyond the limits of nervous impulses; it has been carried deep down into the very innermost molecular processes of life. The closer we penetrate into the physical-chemical events through which living matter grows, lives and dies, the clearer does it seem that life itself is a shifting outcome of two opposing sets of changes—one synthetic constructive, the other destructive, analytic, and that the key to this and that riddle of vital action lies within the grasp of him who can clearly lay hold of the mutual relations of these conflicting changes. The story of the vagus and the heart is a tale, not of the heart alone, not of the nervous system alone, but of all living matter. The light which first shone in the experiment of the brothers Weber may in a sense be said to have gone out into all the lands of physiology.

Let me now turn your attention to an experiment made a few years later. This is also an experiment made on a living animal, and whatever good may have come out of that to which it has given rise must be reckoned as the fruit of an experiment.

In 1851, Claude Bernard made known that division of the cervical sympathetic led to a widening of the blood-vessels and a warming of the ear and other parts of the head and neck. This was the beginning of what may rightly be called the great vaso-motor knowledge. It may be true that more than a hundred years before, in 1727, Du Petit had observed much the same thing, but nothing came out of it; the germinal time had not yet arrived. It may be true that other observers since Du Petit had divided the cervical sympathetic, and noted the effects; but these had their attention directed chiefly to changes in the pupil. It may be true that Brown-Séquard and Waller, a few months before Bernard himself was able to do so, supplied the complement to the original experiment by showing that stimulation of the peripheral part of the divided sympathetic constricted the blood-vessels and reduced the temperature. All this may be true, but there remains the fact that with Bernard's experiment the new light began; that experiment marks the beginning of our vaso-motor knowledge.

I have already spoken of the prolonged discussions, which just before the date of Huxley's studentship were taking place, touching the question whether or no the blood-vessels were muscular and contractile. That question had, meanwhile, been definitely settled by Henle's demonstration, in 1840, that the tissue in the middle coats of arteries really consisted in part of muscular tissue, of the kind known henceforwards as plain muscular tissue. But for some years no use was made of this discovery in the direction of explaining the intervention of the nervous system in the government of the circulation. That began with Bernard's experiment.

It would, I venture to think, be sheer waste of your time and mine, if I were to attempt to labour the theme of the large share in our total physiological knowledge which is now taken up by the vaso-motor system and all that belongs to it, and of the extent to which the physiology of that system has woven itself into pathological doctrines, and helped medical practice. I would simply ask the lecturer on physiology in what stress he would find himself if he were forbidden in his teaching to say a word which would imply that the calibre of the blood-vessels was influenced by the contraction of their walls through nervous influence; or ask the student how often, in an examination of to-day, he would have to sit seeking inspiration by biting his pen, or staring at the roof, if he too, in his answers, could never refer to vaso-motor actions. Whatever part of physiology we touch, be it the work done by a muscle, be it the various kinds of secretive labour, be it that maintenance of bodily temperature which is a condition of bodily activity, be it the keeping of the brain's well-being in the midst of the hydrostatic vicissitudes to which daily life subjects it—in all these, as in many others, we find vaso-motor factors intervening; and, to say nothing of the share taken by these in the great general pathological conditions of inflammation and fever, they also have to be taken account of by the doctor in studying the disordered physiological processes which constitute disease, whatever be the tissue affected by the morbid conditions. Take away from the physiological and pathological doctrines of to-day all that is meant by the word vaso-motor, and those doctrines would be left for the most part a muddled unintelligible mass. To so great an extent as that which Bernard's experiment began entered into our modern views.

It was Bernard's good fortune, but deserved good fortune, to announce, almost at the same time, two fundamental discoveries. For I venture to claim for his discovery of the formation of glycogen in the liver, briefly indicated in 1850, more fully expounded in 1851, an importance only second, if second, to that of the experiment with which we have just been dealing.

To judge of its importance we must look at it from more than one point of view.

At the time when Huxley was sitting at the feet of Wharton Jones, the teaching of the Schools was largely governed by the view that the animal organism, in contradistinction to the vegetable organism, was essentially destructive in its chemical actions, possessing no power in itself of synthetic construction. It is true that the possible synthesis of organic compounds special to the animal body had long before, in 1828, been shown by Wöhler's artificial formation of urea. It is true also that Huber, in the case of bees, and Liebig, in the case of cows, had already shown that wax and fat must be in part manufactured out of something that was not fat. The conclusions, however, of these observers were at best somewhat distant inferences from statistical data; and, in any case, had not as yet made much way in the direction of general acceptance. But Bernard's experiment was in the form of an ocular demonstration. The glycogen which had been formed in the liver could be extracted, could be seen, handled, and, if need be, tasted, a result adequate to convince even a physiological Thomas. We may claim for Bernard's glycogen discovery that as the first realistic proof of the synthetic powers of the animal organism it did much to establish a truth, which succeeding observations have only served to confirm and extend, namely, that the animal, no less than the vegetable organism, possesses synthetic powers, and that the want of prominence of these in the ordinary work of the animal body is to be attributed to economic reasons, and not to absence, or even scantiness of power.

But there is another aspect from which the discovery must be viewed.

At the time of which we are speaking, physiologists were still, as they had been of old, largely under the influence of a somewhat mechanical conception of the body as a collection of organs, each of which had its special use or function, the unity of the body being maintained by the mutual adaptation of the constituent organs. This was further developed into the view that when a use of an organ had been satisfactorily made out, when a function had been made clear, all that remained to be done, in the way of research, was simply to inquire how far and in what ways the performance of that function was influenced by changes in the rest of the body, or by external circumstances. It was acknowledged, for instance, on all hands that the function of the liver was to secrete bile, and physiologists in general were content to look forward for future discoveries which should throw light on the exact nature of the mechanism of the secretion, and on why the liver secreted now more, now less bile, and to these alone without expecting anything else.

Bernard's discovery that the liver not only secreted bile, but manufactured glycogen, fell on physiologists like a bolt from the blue. The knowledge that the same hepatic cell was engaged both in secreting bile and manufacturing glycogen, and that the sugar or other products of digestion were carried from the intestine, not straight to the tissues which they were destined, in any case, ultimately to nourish, but to the liver, there to undergo transformation and await some future fate, marked the beginning of a new way of looking at the problems of nutrition. It was recognised that these became less simple, more complex than they had formerly seemed; but the very complexity gave hope of possible solutions. It was seen that as the blood swept in the blood stream through the several tissues, it might undergo profound changes without any visible outward token, such as that of the appearance of secretion in the duct of a gland, or of the contraction of a muscle, might undergo changes which could only be demonstrated by differences in the composition or properties of the blood as it came to or left this or that tissue. The technical difficulties of the analysis of blood prevented any immediate marked steps in the way of advance, and attempts to establish, in respect to any particular tissue, the changes which the blood underwent in it, by inference from the results of experimental interference, met with difficulties of another but no less serious kind. Hence the world had to wait some little time before the new idea which Bernard's discovery had started bore important or striking fruit. Yet it was not very long before it was seen that the hepatic cell had heavy duties touching the metabolic changes of proteid, as well as a carbohydrate material; that it, and not the kidney alone, had to do with urea as well as sugar, and the difficulties, which physiologists in the early half of this century must have keenly felt, how to reconcile the bald task of secreting bile, which alone technical physiology allotted to the liver, with the overweening importance which not only popular experience, but more exact clinical study, could not but attach to that organ, began to steal away. A little later on, exact experimental inquiry converted into certainty the suspicions which clinical study had raised, that the blood in streaming through the thyroid gland underwent changes of supreme importance to the nutrition of the tissues of the body at large. Still, a little later, the Bernardian idea, if I may so venture to call it, doubling, so to speak, on itself, led to the discovery that the mysteries of the fate of sugar in the body were not lodged in the liver alone, but might be traced to the pancreas. It was seen that as the blood streaming through the liver worked on sugar, besides secreting bile, so the pancreas, besides secreting its marvellous omnipotent juice, also influenced, though in a different way, the career of sugar in the body, that the disease we call diabetes was or might be in some way connected with the pancreas no less than with the liver. I need not go on to speak of recent researches on the supra-renal capsules or of other organs. It is enough to note that one of the most promising lines of inquiry at the present day is that relating to the changes of which I am speaking, sometimes known under the name of "internal secretion." Every year, nay, almost every month, brings up some new light as to the details of the great chemical fight which the blood is carrying on in all the tissues of the body—it may be perhaps to-morrow that we shall learn of some work of a kind wholly unexpected which is carried out by that great Malpighian layer of the skin which wraps round our whole frame. In any case, the line of inquiry is one of the most fruitful of those of the present day. I may add too, I think, that it is one which has been of the greatest direct use to mankind, and promises still more. It is true that Bernard's discovery of glycogen, and perhaps especially the diabetic puncture, raised hopes which have not been fulfilled. Not to-day, any more than forty years ago, it is in our power wholly to remove the disease which we call diabetes. But short of complete mastery, how great is our power now compared with then. And when we remember that the pancreatic relations of sugar are far from being worked out, and that such knowledge as physiologists already possess has not yet made much way in clinical study, we may look forward to marked progress possibly in no very distant time.

Further, if there be any truth in what I have insisted upon—that the value of a discovery is to be measured not only by its immediate application, theoretical and practical, but also by the worth of the idea which it embodies and to which it gives life; and if it be true, as I have suggested, that by the genesis of ideas the discovery of glycogen is mother of all our knowledge of internal secretion, in its widest sense, of the work of the thyroid and other like bodies, then the good to suffering mankind which may be laid to the door of Bernard's initial experiment is great indeed.

The next result to which I will call your attention is again an experiment, and once more an experiment on a living animal. In 1850, Augustus Waller described, in the *Philosophical Transactions*, the histological changes which division of the hypoglossal and glosso-pharyngeal nerves in the frog produced in the fibres of the distal portions of the nerves, and shortly afterwards developed this initial result into the more general view of the dependence of the nutrition of a nerve-fibre on its continuity with a cell in the central nervous system, or in the case of afferent fibres, in the ganglion of the posterior root.

This discovery was at the time, and has since continued to be of value as a contribution to physiological ideas; it had its share in promoting the progress which, though slight, is still a progress, of our understanding the obscure influences which the part of a cell enclosing the mysterious nucleus exercises over all the rest of the cell. And perhaps even to-day the theoretical value of that degeneration of nerve-fibres, the knowledge of which we owe to Waller, is not adequately appreciated, and the lead which it gives not followed out as it might be. In spite of all we know, we are too much apt to fall back on the conception that, when no nervous impulse is travelling along a nerve-fibre, the nerve-fibre is in a state of motionless quiescence, and that a nervous impulse, when it does come, sweeps over the fibre as a wave sweeps over a placid lake. But the Wallerian degeneration gives such a view the lie direct. When we reflect that the finely-balanced molecular condition, which itself is nothing more than the falsely seeming quiescence of an equilibrium of opposing motions, in the ultimate fibrils of the nerve-twigs, in the ultimate phalanx of the finger, by which we touch and get to know the world without us, is dependent on what is going on around the nucleus of a cell, or the nuclei of some cells in the ganglion or ganglia of certain upper spinal nerves, so that if the continuity of the axis cylinder process be anywhere broken, the figure of the molecular dance changes at once, and riot takes the place of order. When we reflect on this, it is clear, I say, that between the molecules of the ultimate fibrils branching in the Malpighian layer of the ball of the finger, and the molecules within the immediate grasp of the nucleus of the cell from which those fibrils start, there must be ever-passing thrills—thrills, it is true, of so gentle a kind, that no physical instrument we as yet possess can give us warning of them, so gentle, that compared with them, the wave, which carries what we call a nervous impulse, must appear a roaring avalanche, but still thrills the token of continued movement. And of such gentle impalpable unnoticed thrills, we must in the future take full account, if we are ever to sound the real depths of nervous actions.

It is not, however, as a contribution to theoretical conceptions, but rather as a method, that the results of Waller have so far had their chief effect on the progress of physiology and medicine. And I have chosen it as a thing to dwell on, because it seems to me a striking instance of the value of a method merely judged as a method, and, further, because the value of its use illustrates my theme that the success of any one scientific effort is contingent on the converging aid of other efforts. For some time, it is true—for years, in fact—the Wallerian method was employed solely or chiefly in what, without reproach, may be called the smaller problems of physiology; it settled many topographical questions, it cleared our views as to the distribution of afferent and efferent fibres; it seemed to add or replace a few stones here and there in the growing building, but it did not greatly change the whole edifice. After a while, however, it met with two helpmates—the one sooner, the other later—and, by means of the three together, we have gained, and are still gaining such additions to our knowledge of the ways in which the central nervous system works out the acts which make up our real life, as to constitute perhaps the most striking progress in the physiology of our time. A wholly new chapter of nervous physiology has, through them, been opened up.

The one colleague is to be found in the experiments of Fritz and Hitzig; and of Ferrier, again, experiments on living animals—experiments which, by demonstrating the existence of definite paths for the play of nervous impulses within the central nervous system, opened up paths for the play of new ideas concerning the working of that system. I say "demonstrating the existence of definite paths," for this—and not the topographical recognition of so many centres of hypothetical nature—is the solid outcome of experiments on local stimulation of the cerebral cortex. Views come and go as to what is happening when the current is flitting to and fro between two electrodes placed on a particular spot of the Rolandic area; the solid ground on which each view strives to establish itself is, that the particular spot is joined by definite nervous paths to particular peripheral parts. I say "demonstrating the existence of particular paths," but what would have been the demonstrative value of the experiments of stimulation, or of removal, by themselves, without the anatomical support furnished by the Wallerian method? And I may justly include within the Wallerian method, not the mere tracking out the degenerated fibre by the simple means at Waller's own disposal, but such finer, surer search as is afforded by the later help given by the newer development of the staining technique.

They who have the widest experience of experiments on living animals are the first to own that in a region of delicate complexity like that of the central nervous system, the interpretation of the results of any experimental interference may be, and generally is, in the absence of aid from other sources, a matter of extremest difficulty, one in which the observer, trusting to the experiment alone, may easily be led astray. I need not labour the question what would have been the value of the mere effects of stimulating or even of removal of parts of the cerebral cortex, and whither would they have led us, had the experimental results not been supported and their interpretation guided by the teachings of the Wallerian method. It is not too much to say that the experiments of Ferrier and his peers, brilliant as they were, might have remained barren, useful only as isolated bits of knowledge, or might even have led us astray, had they not been complemented by anatomical facts. They have not remained barren, and they have not led us astray. The Wallerian method picked out from the tangle of nerve fibres making up the white matter of the brain and spinal cord, the pyramidal tract running from the Rolandic area, to the origins of all the motor roots, even of the lowest, and so joining hands with the experiment, made it clear that whatever might be the exact nature of the events taking place in a particular spot of the cortex of that area, that spot was, by the definite paths of particular nerve fibres, put in connection with definite skeletal muscles. The pyramidal tract was further shown to be merely one—an important one, it is true—but still merely one of a large class. So it is that the experimental results and the Wallerian results, not merely in that Rolandic area where the results of experiment take on the grosser form of readily appreciated interference with movements, but in other regions where other finer, more occult manifestations of nervous and psychical actions have to be dealt with are, it may be slowly, but yet surely, resolving that which seemed to be a hopeless tangle of interweaving and interlacing nerve fibres and cells, into an orderly arrangement of which the key is seen to be that each nerve filament is a path of impulses coming from some spot—it may be from near, it may be from afar—where events are taking place, and carrying the issue of those events to some other spot, there to give rise to events having some other issue.

But a third factor was wanting to forward our insight into this orderly arrangement, and especially by again affording an anatomical basis to open the way towards explaining what was the order of events in the spots or centres, as we call them, in which the filaments began or ended, and what was the mechanism of the change of events. This, I venture to think, we may find in the special histological method which, however much its usefulness, has been enhanced by its subsequent development in the hands of Cayal, Kölliker, and others, as well as by the coincident methyl-blue method we owe to Golgi. The final word has not yet been said as to the exact meaning and value of the black silver pictures which that method places before us; but this, at least, may be asserted that by means of them the progress of our knowledge of the histological constitution of the central nervous system has within the last few years made strides of a most remarkable kind. It may be that those pictures are in some of their features misleading, it may be that the terminal arborisation, and their lack of continuity with the material of the structures which they grasp, does not afford an adequate explanation of the change in the nature of the nervous impulses which takes place at the relays of which the arborisations seem the token; it may be, indeed it is probable, that we have yet much to learn on these points. But notwithstanding this, it must still be said that, by the help of this method, our knowledge of how the fibres run, where they begin and where they end within the brain and spinal cord, has advanced, and is advancing in a manner which, to one who looks back to the days when Huxley was studying within these walls, seems little short of marvellous.

Let me once more repeat, the value of this silver method is not an intrinsic one, it has its worth because it fits in with other methods, it is available on account of what is known apart from it. I imagine that if in 1842 Huxley, at Wharton Jones' suggestion, had invented the silver method, it would have remained unknown and unused. The time for it had not then come. The full fruition which it has borne, and is bearing in our day has come to it, because it works hand in hand with the two other methods, of which I have spoken—the Wallerian and the experimental method.

It is these three working together which have brought forth what I may venture to call the wonders which we have seen in our days, and I cannot but think that what we have seen is but an earnest of that which is to come. In no branch of physiology is the outlook more promising, even in the immediate future, than in that of the central nervous system. But surely I do it wrong to call it merely a branch of physiology. It is true that if we judge it by even the advanced knowledge of to-day, it takes up but a small part of the whole teaching of the science; but when we come to know about it that which we are to know, all the rest of physiology will shrink into a mere appendage of it, and the teacher of the future will hurry over all that to which to-day we devote so much of the year's course, in order that he may enter into the real and dominant part.

There is no need for me to expound in detail how the knowledge gained by the three methods, of which I have been speaking, in laying bare the secrets of nervous diseases, and opening up the way for successful treatment accurate and trustworthy prognosis, has helped onward the art of medicine. Even the younger among us must be impressed when he compares what we know to-day of the diseases of the nervous system with what we know, I will not say fifty, but even twenty, nay even ten, years ago. Do not for a moment suppose that I am attempting to maintain that the great clinical progress which has taken place, has resulted from the direct, immediate application to the bedside of laboratory work, or that I wish to use this to exalt the physiological horn. I would desire to take a higher and broader standpoint, namely this, that the close relations and mutual interdependence of laboratory physiology and that bedside physiology, which we sometimes call pathology, and the necessity of both for the medical art, are nowhere more clearly shown than by the history of our recent advance in a knowledge of the nervous system as a whole. In this, when we strive to follow out the genesis of the new truths, it is almost impossible to trace out that which has come from the laboratory and that from the hospital ward, so closely have the two worked together; an idea started at the bedside has again and again been extended, shaped or corrected by experimental results, and been brought back in increased fruitfulness to the bedside. On the other hand, a new observation, which, had it been confined to the laboratory, would have remained barren and without result, has no less often proved in the hands of the physician the key to clinical problems, the unlocking of which has in turn opened up new physiological ideas.

And, though the scope of these Huxley lectures is to deal with the relations of the sciences to the medical art, I shall, I trust, be pardoned if I turn aside to point out that this swelling knowledge of how nerve-cell and nerve-fibre play their parts in bringing about the complex work done by man's nervous system, is not narrowed to the relief of those sufferings which come to humanity in the sick room. Mankind suffers, much more deeply, much more widely, through misdirected activities of the nervous system, the meddling with which lies outside the immediate calling of the doctor. Yet every doctor, I may say every thoughtful man, cannot but recognise that the distinction between a so-called physical and a so-called moral cause is often a shadowy and indistinct one, and that certainly so-called moral results are often the outcome, more or less direct, of so-called physical events. I venture to say that he who realises how strong a grip the physiologist and the physician, working hand in hand, are laying on the secret workings of the nervous system, who realises how step by step the two are seeing their way to understand the chain of events issuing in that sheaf of nervous impulses which is the instrument of what we call a voluntary act, must have hopes that that knowledge will ere long give man power over the issue of those impulses, to an extent of which we have at present no idea. Not the mere mending of a broken brain, but the education, development and guidance of cerebral powers, by the light of a knowledge of cerebral processes, is the office in the—we hope—not far future of the physiology of the times to come.

I might bring before you other illustrations of the theme which I have in hand. I could, I think, show you that the very greatest of all recent advances in our art, that based on our knowledge of the ways and works of minute organisms, has come about because several independent gains of science met, in the fulness of time, and linked themselves together. But my time is spent.

I should be very loth, however, and you, I am sure, would not wish that I should end this first Huxley lecture, without some word as to what the great man, whose name the lectures bear, had to do with the progress, on some points of which I have touched. He had an influence, I think a very great one, upon that progress, though his influence, as is natural, bore most on the progress in this country.

The condition and prospects of physiology in great Britain at the present moment are, I venture to think, save and except the needless bonds which the legislature has placed upon it, better and brighter than they ever have been before. At one time, perhaps, it might have been said that physiology was for the most part being made in Germany; for, in spite of the fact that some of the greatest and most pregnant ideas in physiology have sprung from the English brain, it must be confessed that in the more ordinary researches the output in England has at times not been commensurate with her activities of other kinds. But that cannot be said now. The English physiological work of to-day is, both in quantity and quality, at least equal to that of other nations, having respect to English resources and opportunities. Part of this is probably due to that activity which is the natural response to the stimulus of obstacles; the whip of the antivivisectionists has defeated its own end. But it is also in part due to the influence of Huxley.

That influence was two-fold, direct and indirect. I need not remind you that not only when he sat on the benches of Charing Cross Hospital, but all his life-long afterwards, Huxley was at heart a physiologist. Physiology, the beauty of which Wharton Jones made known to him, was his first love. That Morphology, which circumstances led him to espouse, was but a second love; and though his affection for it grew with long-continued daily communion, and he proved a faithful husband, devoting himself with steadfast energy to her to whom he had been joined, his heart went back again, and especially in the early days, to the love which was not to be his. What he did for morphology may perhaps give us a measure of what he might have done for physiology, had his early hopes been realised. As it was, he could show his leanings chiefly by helping those who were following the career denied to himself. Unable to put his own hand to the plough, he was ever ready to help others, whom fate had brought to that plough, especially us younger ones, to keep the furrow straight. And if I venture to say that the little which he who is now speaking to you has been able to do, is chiefly the result of Huxley's influence and help, it is because that only illustrates what he was doing at many times and in many ways.

His indirect influence was perhaps greater even than his direct.

The man of science, conscious of his own strength, or rather of the strength of that of which he is the instrument, is too often apt to underrate the weight and importance of public opinion, of that which the world at large thinks of his work and ways. Huxley, who had in him the making of a sagacious statesman, never fell into this mistake. Though he felt as keenly as any one the worthlessness of popular judgment upon the value of any one scientific achievement, or as to the right or wrong of any one scientific utterance, he recognised the importance of securing towards science and scientific efforts in general a right attitude of that popular opinion which is, after all, the ultimate appeal in all mundane affairs.

And much of his activity was directed to this end. The time which seemed to some wasted, he looked upon as well spent, when it was used for the purpose of making the people at large understand the worth and reach of science. No part of science did he more constantly and fervently preach to the common folk, than that part which we call physiology. His little work on physiology was written with this view, among others, that by helping to spread a sound knowledge of what physiology was, among the young of all classes, he was preparing the way for a just appreciation among the public of what were the aims of physiology, and how necessary was the due encouragement of it.

And if, as I believe to be the case, physiology stands far higher in public opinion, and if its just ambitions are more clearly appreciated than they were fifty years ago, that is in large measure due to Huxley's words and acts. I have not forgotten that he was one of a Commission whose labours issued in the forging of those chains to which I have referred; but knowing something of Commissions, and bearing in mind what were the views of men of high influence and position at that time, I tremble to think of what might have been the fate of physiology if a wise hand had not made the best of adverse things.

One aspect of Huxley's relations to science deserves, perhaps, special comment. On nothing did he insist, perhaps, more strongly than on the conception that great as are the material benefits which accrue from science, greater still is the intellectual and moral good which it brings to man; and part of his zeal for physiology was based on the conviction that great as is the help which, as the basis of the knowledge of disease, and its applications to the healing art, it offers to suffering humanity in its pains and ills, still greater is the promise which it gives of clearing up the dark problems of human nature, and laying down rules for human conduct. No token, in these present days, is more striking or more mournful than that note of pessimism which is sounded by so many men of letters, in our own land, no less than in others, who, knowing nothing of, take no heed of the ways and aims of science. Cast adrift from old moorings, such men toss about in darkness on the waves of despair. There was no such note from Huxley. He had marked the limits of human knowledge, and had been led to doubt things about which other men are sure, but he never doubted in the worth and growing power of science, and, with a justified optimism, looked forward with confident hope to its being man's help and guide in the days to come.